used, is in the construction of a tube, which *Trigona jaty* Sm. builds at the entrance of its nest.

Among European Apidæ, Apis and Bombus are the only genera which wet with honey the pollen they are collecting, and in consequence of this habit the hairs on the outside of the tibiæ of the hind-legs have disappeared. This is also the case with our Meliponæ, Trigonæ, and Euglossæ. Now Centris, Tetrapediæ, Epicharis, and some other bees, collect pollen in the same way; but notwithstanding, in some species, the hairs on the tibiæ are developed in an extraordinary degree. This seemed to me rather perplexing, till I lately observed several species of Centris and a Tetrapedia gathering sand in the large hair-brushes of the hind-tibiæ, which accounts for the conservation and excessive development of the hairs.

With one of our smallest Trigonæ (*T. mirim* n.sp.), of which I have two hives in my garden, I have made a long series of observations on the construction of the combs, in which the young are raised. As in all other species the combs are horizontal and consist of a single layer of hexagonal cells, like those of wasps; but the cells are vertical. There is always in this species (other species behave differently) a set of cells constructed at the same time in the circumference of the two or three uppermost combs. When the cells are ready, they are filled with food, which the bees vomit from their mouths, the queen lays an egg into every cell and these are then immediately shut. The eggs at first lie horizontally; but in the course of the first or second day they assume a perpendicular position, with the thicker end turned upwards, dipping but slightly into the semi-fluid food. The combs are never used more than once; as soon as the young bees have left them (five to six weeks after the laying of the eggs) they are destroyed and new ones built in their place.

Once I assisted at a curious contest, which took place between the queen and the worker bees in one of my hives, and which throws some light on the intellectual faculties of these animals. A set of 47 cells had been filled, 8 on a nearly completed comb, 35 on the following, and 4 around the first cell of a new comb. When the queen had laid eggs in all the cells of the two older combs she went several times round their circumference (as she always does in order to ascertain whether she has not forgotten any cell), and then prepared to retreat into the lower part of the breeding room. But as she had overlooked the four cells of the new comb the workers ran impatiently from this part to the queen, pushing her, in an odd manner, with their heads, as they did also other workers they met with. In consequence the queen began again to go around on the two older combs, but as she did not find any cell wanting an egg she tried to descend; but everywhere she was pushed back by the workers. This contest lasted for a rather long while, till at last the queen escaped without having completed her work. Thus the workers knew how to advise the queen that something was as yet to be done, but they knew not how to show her *where* it had to be done. In the same hive there appeared to be two political parties among the workers, dissenting about the construction of the combs, one destroying what the other had begun to build; but it would require a very long and tedious exposition to give you the details of the case.

Our several species of honey-bees differ as much in their mental dispositions as they do in external appearance and size (the smallest species, called *Trigona lilliput* by my brother, is only about 2·5 mm. long). Some rush furiously out of their nest, whenever an enemy approaches it, attacking and persecuting the offender; others are very tame, and permit close observation of all their work. In one large species I could even observe with a lens the act of their sucking a solution of sugar, which I had given them, and there was no doubt that at least these bees really suck, and do not lap, like dogs or cats, as Milne Edwards, Gerstäcker, and most entomologists think.

There is one species (*Trigona liomão* Sm., named for my brother by Mr. Frederick Smith himself) which never appears to collect honey or pollen from flowers, on which, at least, I have never seen it. It robs other species of their provisions and sometimes takes possession of their nests, killing or expelling the owners. The hives in my garden have often been invaded, and two of them destroyed, by these robbers, and I have seen in the forest several nests, formerly inhabited by other species, occupied by them.

Together with my brother at Lippstadt I intended to publish an essay on the natural history of our stingless honey-bees, but it will probably cost some years to give a tolerably complete account of them.

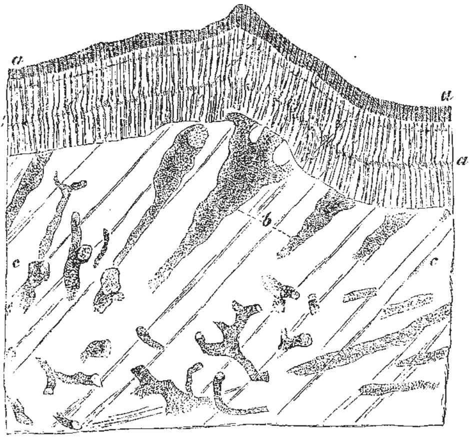
Fritz Müller

Itajahy, Santa Catharina, Brazil, April 20

Eozoön canadense

I desire permission to state, in your journal, my entire agreement with the explanation of the actual structure of this fossil given by Dr. Carpenter in the Ann. Nat. Hist. for April. Though it may not be necessary to corroborate, in any way, the decisions of so great an authority on *Foraminifera*, or to add to illustrations so clear and convincing, my testimony may not be without its value; since, in addition to work in micro-geology extended over more than thirty years, and some familiarity with modern *Foraminifera*, I have, in the original examination of *Eozoön*, undertaken at the request of Sir William Logan, studied larger suites of specimens of typical *Eozoön*, and of materials supposed to resemble it, not only from Canada, but from other localities, than any other person.

I have the more pleasure in bearing testimony to the "tubulated primitive chamber-wall," because this was not manifest in my original specimens, and was first made out by Dr. Carpenter in those submitted to him from *Petite Nation* after my original description was written. I did not, however, take it for granted even on Dr. C.'s testimony, but satisfied myself of the organic nature of the structure by careful examination and comparison with the Chrysotile and other fibrous minerals occurring in connection with some of the specimens.



Part of a Calcareous lamella of *Eozoön canadense*, showing at *a a* the tubulated structure of the proper wall of the chamber or "nummuline layer," perfectly differentiated from the serpentine chamber-cast on which it abuts, and at *a' a'* a line of flexure of the tubuli, corresponding with that often seen in dentine and other tubulated calcareous structures; *b*, origins of the "canal system" in irregular lacunæ of the "intermediate skeleton" on the exterior of the proper wall of the chamber, precisely as in *Calcarina*; *c*, *c*, "intermediate skeleton," traversed by cleavage-planes, whose extension into the "nummuline layer" proves it to be a part of the calcareous, not of the serpentinous, lamella.—From a figure given by Dr. Carpenter from the Ann. Nat. Hist. for June.

It is not surprising that *Eozoön* mets with some opponents. There are few naturalists who have sufficient familiarity with the structures of modern *Foraminifera*, and with those strange and gigantic representatives of the *Protozoa* found in the Primordial and Silurian rocks, to appreciate the importance of the structures it presents. Still fewer have added to this experience by the study of the structures of the fossils of the more ancient rocks as they appear under the microscope, and of the conditions of mineralisation of such fossils. The intelligent appreciation of the claims of *Eozoön* must, therefore, be of slow growth; and the controversies respecting it will be finally settled only when the other organisms of which traces exist in the Laurentian rocks are better understood, and when the *Protozoa* of the Cambrian and Silurian have been more thoroughly investigated. These desiderata are gradually being supplied; and I venture to predict that before many years have passed, palæontologists will be required to extend their belief to several other Laurentian and Primordial *Foraminifera* besides *Eozoön canadense* and *Eozoön bavaricum*.

J. W. Dawson

McGill College, Montreal, May 15

Proportionality of Cause and Effect

It does not surprise me that Mr. Hayward gives up in despair the attempt to make Mr. Spencer conscious of the fallacies in his logic. But as from the first I have addressed myself to Mr. Spencer's readers, I must in justice to myself point out to them the true

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nature of the controversy in order to counteract the effect of Mr. Spencer's endeavours to represent it as a controversy between those who think that forms of thought become hereditary and those who do not. The original attack centred upon the fallacious character of certain would-be *à priori* proofs of physical laws. Mr. Spencer has tried to parry the attack by maintaining that the writer misunderstood the sense in which the phrase *à priori* was used. That the new interpretation was not the one which it was at the time intended to bear is rendered as clear as the English language permits by his speaking of one of these truths as *not* resulting "from a long registry of experiences gradually organised into an irreversible mode of thought," and his using similar, or even stronger expressions of the others. But this is, after all, not the real issue. No definition of *à priori* would cure the fallacies in the proofs in question or in the subsequent attempts that he has made to support them. They are as illogical with the one definition as with the other; and the sole result of Mr. Spencer's change of front will be, I think, to supply the critics of his writings on Physics with another instance of his habit of changing the meanings of the terms he employs without perceiving that by so doing he forfeits the right to use previous conclusions, even though legitimately obtained, and destroys all connection between the bases and the later parts of his system.

As I have already said, I have been chiefly addressing myself to Mr. Spencer's readers. My aim has been to show that his writings on Physics are marred by superficial and inconsistent views of the subject-matter and fallacies in reasoning thereon. I have been accused of being too violent in my language, and some of my friends have urged, like Mr. Hayward, that it would have been better had I used expressions which less adequately conveyed my (and their) opinion of the magnitude of the errors I was attacking. As I have left the department of personal abuse wholly to Mr. Spencer, I do not think he has much right to complain, even though I have not hesitated to call absurdities by what seemed to me descriptive and suitable titles, and I will conclude this by calling attention to a last effort by Mr. Spencer to show that there is some excuse for expressive language on my part, provided always it is directed to the blunder rather than to the blunderer.

IN NATURE, vol. ix. p. 461, Mr. Spencer asserted that the second law of motion was a mere corollary from the general postulate that cause and effect are necessarily connected together, and in all cases by definite quantitative relations. As every mathematician will at once see that there is a great difference between asserting that there is *some* definite relation between cause and effect and asserting that this relation is the *particular one* of direct proportionality, it will be asked how he came to consider the one a mere deduction from the other? It will be seen, on examination of the passage, that he is misled by a couple of instances that he cites (and of course he might have cited countless others), where there is this simple relation between a prominent part of the cause and a prominent part of the effect. The fallacy of this was pointed out by a writer who signed himself "A Senior Wrangler" in the next number of NATURE, and to this Mr. Spencer replies in the number for May 7:—

"Nor should I care to discuss any question with my new anonymous assailant, who, when certain examples given *show the 'exact quantitative relations spoken of to be those of direct proportion,'* describes me as 'intensely unmathematical,' because I subsequently use the more general expression as equivalent to the more special—which, *in the case in question*, it is."

Now, in the first place, the phrase "certain examples show," amounts to admitting that the argument is inductive in its nature, which is inconsistent, to say the least, with the professions he makes, for the proof is not only not to be an inductive one, but is to render it clear that no such proof of the matter in question could possibly exist; but this is a trifle to that which follows it. Can anyone avoid admitting that the italicised words leave Mr. Spencer committed to at least one of the following propositions:—

1. That these (and similar) instances establish the proposition that the 'exact quantitative relations' between cause and effect are, *in all cases*, those of direct proportionality.

2. That in a proof (other than an inductive one) you may assume the result during the progress of the argument without invalidating the proof.

The first of these is saved from being pronounced contrary to fact by being discovered, on closer examination, to be meaningless; nothing but the most superficial notions of the meanings of the words cause and effect can prevent its being seen to be unmeaning. The second is too common a logical error to need exposing. What examiner in Euclid has not rejected attempts at the solution of geometrical deductions for this fallacy? If a boy has to prove a triangle to be equilateral, cruel mathematicians do not allow him to assume that it is so in the course of his proof. But Mr. Spencer would take a more lenient view of the matter, and would allow him to use "the more general expression (*i.e.* triangle) as equivalent to the more special (*i.e.* equilateral triangle), which, in *the case in question*, it is."

THE AUTHOR OF THE ARTICLE ON HERBERT SPENCER
IN THE BRITISH QUARTERLY REVIEW.

MR. COLLIER, in his anxiety to "transfix" me on one of the horns of a dilemma, has shown himself strangely blind to the fact that he could only do so by thrusting at me through the body of his leader, Mr. Spencer. My wound is consequently but skin deep; but what of Mr. Spencer's?

As I have carefully avoided representing Mr. Spencer otherwise than by quoting his own words, the charge of "misrepresentation" (an ugly word, which, I think, Mr. Collier, on reconsideration, will regret having used) falls to the ground; and if, as Mr. Collier clearly enough shows, there be inconsistency in the phraseology used by Mr. Spencer at different times, the responsibility rests with Mr. Spencer and not with me.

The facts are briefly these:—Mr. Spencer first asserted the Second Law of Motion to be an "immediate *corollary of the preconception*," &c. I criticised the assertion. Mr. Spencer characterised my criticism as a proposal to "exemplify *unconsciously-formed preconceptions*." I did not care for the moment to quarrel with this description lest I should multiply and thus "confuse the issues" between us; and so adopting the phrase under the safeguard (insufficient as it now appears to have been, at least for Mr. Collier) of the usual marks of quotation, I noted what appeared to me an admission, implied in Mr. Spencer's remarks, and important as bearing on the real issue between us, that the Second Law of Motion is a "*consciously-formed hypothesis.*" Mr. Collier has done well in calling attention to the discrepancy between the first two phrases italicised. He might also have noted the discrepancy between both of these and the third. But the phrases are Mr. Spencer's; and the only crime to which I can plead guilty is that of not having seen the necessity of more explicitly repudiating Mr. Spencer's characterisation of my criticism, and thus saved Mr. Collier from bringing charges against me of "confusing issues," &c., which I can only transfer to Mr. Spencer.

And now having cleared the path of the personal questions which Mr. Collier has raised, I would appeal to him to obtain for me and other perplexed readers of NATURE an authoritative statement as to what Mr. Spencer's latest views as to the Second Law of Motion are. Does Mr. Spencer regard it as an "unconsciously-formed preconception," or as a "corollary of a preconception," or as a "consciously-formed hypothesis?" Each of these views seems to be deducible from Mr. Spencer's language, but I agree with Mr. Collier that they can hardly be regarded as one and the same thing.

I would also remind Mr. Collier that no answer has yet been given to the difficulties which in my first note I showed to attach to the view of the Second Law of Motion as a "corollary of a particular preconception;" and that, unless Mr. Spencer, or Mr. Collier on his behalf, can show that these difficulties are imaginary, judgment will be recorded against them by default by all readers of NATURE who have had patience to follow the controversy thus far.

ROBT. B. HAYWARD

Harrow, June 6

I OUGHT to thank Mr. Collier for the care with which he has explained his previous letter, but to assure him at the same time that I fully understood it before; his italics have only made plain what was accurately and lucidly expressed before, and have only served to convince me that I thoroughly understand his position, and that it is wholly untenable.

I will make one more effort to show this, by pointing out one of the fallacies in Mr. Collier's last letter. He says "Mr. Spencer alleges that this cognition of proportionality is *à priori*; his opponents affirm that this cognition is *à posteriori*."

The "cognition" spoken of is not one, but two. Mr. Spencer alleges that a conviction of a quantitative relation of some kind between cause and effect, such that the greater cause produces the greater effect, grows in our minds from experiences which are antecedent to reasoning. No one denies it. But to call this a cognition of *proportionality* is so utterly inaccurate an expression as to astound me. And the consequences of the inaccuracy

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